

Final Report on Grant No. N00014-95-1-0596

"Study of High Temperature Superconductors Using Hydrostatic Pressure and Uniaxial Stress Simultaneously: First True Uniaxial Stress Measurements."

Grant Period: September 1, 1995 - December 31, 1996

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Though we were not able to achieve the objectives of the proposed research in the grant period requested, we have made progress in the testing of this new type of equipment which allowed finally the first measurements on semiconductor lasers under "*true*" uniaxial stress. At the moment we are preparing a manuscript describing the design and the results of these first measurements which will be submitted for publication in Rev. Sci. Instrum. Furthermore, we have designed and tested a new sapphire anvil cell, which has allowed us to obtain necessary information for the compensation of the Poisson ratio for the measurements on high T_c materials using energy dispersive X-ray diffraction at high pressure. The results of these measurements will be published in the Journal of Physics and Chemistry of Solids in 1997. (K. Weishaupt, J. Th. Held, H. D. Hochheimer, S. B. Quadri, E. F. Skelton, K. Brister, "Equation of State of $\text{PrBa}_2\text{Cu}_3\text{O}_{7.5}$ "

J. Phys. Chem. Solids, 1997)

In the following I will present a report on the status of the project and elaborate on some of the unexpected difficulties and problems which we have encountered, the solutions which we have found, and what we still have to do.

We have put an incredible amount of work in the project, gained enormous insights and have made considerable progress, but unfortunately not to the point, that we could have done reproducible reliable measurements.

In March of 1996 we had tested the equipment and achieved a temperature of 127 K.

I thought it save enough to project that we could achieve the temperature for the measurements using for the last decrease liquid helium. When Marc Raphael, a graduate student of Professor Reeves, George Washington University, who joined in the project came to Colorado State University to work with us, he improved the insulation and contact in such a way that we got down to a temperature of 110 K with liquid nitrogen as a coolant. We discovered, however, that a leak occurred at low temperatures, and Earl Skelton, Mark Reeves, and I decided to solve this problem before we move the equipment to NRL. In the meanwhile my student working on the project decided to leave because of the insecure funding situation. Marc Raphael and I as well as other students of my group continued to work on the problem. At the same time we had to modify the closure plug of the cell for the measurements on the superconductor. After we had the new plug we put the electrical feedthroughs in, which suddenly did not seal anymore. Now we were sitting here with two plugs with working electrical feedthroughs which we could not use for the experiments, and an appropriate plug with leaking feedthroughs. After 4 weeks we finally found the culprit. The problems were caused by the new cones, which did not meet the specifications and by the pyrophyllite powder, which had lumps in it which scratched the cones. Problems were also caused by the insulation of the thermocouple wires, which had a teflon

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insulation. All attempts to buy lacquer insulated thermocouple wires failed here in the States, but I have found a source in Germany, where we could order them. Unfortunately with a delivery time of 8 weeks. We had also problems to find someone to machine the precision parts and packings here. Again I got help from former colleagues in Germany.

I also made an arrangement with the Abt. fuer Angewandte Physik in Ulm. They have fabricated high precision parts for us in exchange for my consulting to build up an optical high pressure system there.

Mark Reeves could solve the problem with the pyrophyllite by finding someone at NRL who could powder it ultrasonically. I could convince one of our German mechanical engineering students, who just finished his practical training with me, to stay another week and make new cones. All this resulted in producing a helium leak tight plug for the measurements. In the meanwhile I had also some new packings, which we could test. They sealed absolutely at room temperature indicated by an order of magnitude better vacuum. This vacuum even improved when we started cooling down, but at 170 K a sudden leak occurred. Warming up only slightly caused sealing again. This was the worst situation possible, because now we cannot easily detect the leak and fix it. It is clear that it is one of the packings, which loses its ability to deform at this temperature enough to provide sealing. We have, however, 16 packings in the system, and we can find the culprit only by testing them one by one. As each test includes assembly and disassembly of the cell and the cryostat as well as cooling and warming up it takes about 3 days. At that time classes had started and I could not spend 12-14 hours in the lab anymore as I did during summer. Furthermore, the funding was running out so that I could not hire additional help. These facts led after consultation with Earl Skelton and Mark Reeves to the decision to cancel the planned move to NRL in order to have time enough to solve this problem. I have tried to give an honest report where we stand at the moment. I have pointed out the problems and the solutions which we have found and indicated the problems which still have to be solved. Though we could not achieve the proposed objectives we have made tremendous progress and everybody involved has learned a lot. The status of the project at the moment will allow us to do the proposed measurements with a relatively low funding level. Therefore, I will apply for funding again, as soon as we have solved the intermittent leak problem.

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